nature portfolio

Peer Review File

Caves demonstrate decrease in rainfall recharge of groundwater is unprecedented for the last 800 years

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

21st Jun 22

Dear Dr Priestley,

Your manuscript titled "Caves provide early warning of unprecedented decrease in rainfall recharge of groundwater" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

In particular, please ensure that the revised manuscript meets the following editorial thresholds:

Present a robust and compelling case for a substantial rise in d18O within the southwest Australian region

Provide a thorough explanation of the conceptual models of flow pathways and recharge you use to support your conclusions including a clear discussion of the associated limitations and uncertainties

Provide additional evidence to support your interpretations for the rise in d18O or soften caveat these conclusions accordingly

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Olga Churakova, PhD

Editorial Board Member

Communications Earth & Environment

0000-0002-1687-1201

Joe Aslin

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EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

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For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of Manuscript COMMSENV-22-0366-T entitled “Caves provide early warning of unprecedented decrease in rainfall recharge of groundwater”.

In the manuscript, the authors presented new and previously published d18O data of speleothem from five caves along southwestern Australia coast. Authors combined speleothem records with farmed calcite, drip water d18O data from one of the caves, and presented a composite speleothem record for 1910- 2020, which showed ~1‰ uptick in d18O values since 1990s. Authors argued that this positive change is due to the reduced fracture flow (lower d18O values due to amount effect) vs matrix flow (higher d18O values), as a result of reduced rainfall recharge. Authors further stated that this change in speleothem d18O values indicates reduced recharge to groundwater from the rainfall in the region.

Considering the numerous speleothems within each individual cave, it is not an easy task to acquire multiple speleothem samples from different caves across a large region that overlap a common growth period. They carry important “big picture view” of the regional climate and improve our understanding of regional and global climate forcing. As such, this study is important, creative, and warrants the publication.

There are some significant issues, however, I would like authors to address. They are summarized below.

First, a minor problem is that it would seem there is no age constrain on Golgotha speleothems for the duration presented in the study (1910-2020). Treble et al (2022), ref 26, showed that dated layers for these speleothems from Golgotha are all older than 1910. Considering milling increment of 100-200μm and very short total milling length of these samples in Fig. S1 (<1cm), each sample's age would have some significant uncertainties. It would be informative to add these error bars onto Fig. 2.

Second, regarding to the observation of d18O uptick since 1990s. This doesn't seem to be prominent in sample GL-S1, YD-S2, GL-S2, MND-S1. Furthermore, if farmed calcite and dripwater derived calcite d18O is used as part of the record to produce this "uptick", they are only from one cave. It is also risky to combine farmed modern calcite with speleothem calcite for a composite record, as there are plenty unknowns regarding potential differences between these two, the risk goes even higher when we derive calcite d18O values from drip water d18O values using a fractionation factor that may not have been the same during the growth of the speleothems.

Thirdly, with the d18O uptick established, the difference in d18O values between diffuse/matrix and fracture/conduit flow path seems an inadequate explanation for the 1‰ uptick. Supp table 4 in ref 26 describes a difference as small as 0.26‰, with the largest difference from sept-13 being only 0.67‰ when uncertainty is included. This contrasts with uptick of 1‰+ shown in Fig. 2.

Fourth, the impact of evaporation of water along matrix flow path, and kinetic effect on calcite d18O values were not adequately addressed. They could be assessed using additional data that may already exist.

If matrix flow water is 1‰ more positive than ground water/fracture flow, could it be due to prolonged residence time of these slow-moving water in vadose zone and their susceptibility to evaporation before reaching the caves? If so, could marked reduction in precipitation in the region post 1990s have led to more positive matrix water d18O values that contributes to uptick in calcite d18O? Additional dD vs d18O plot of rainfall, matrix drip water, along with GMWL and LMWL could help clarify this.

Kinetic fractionation due to variable growth rate (in mm/yr) of speleothems could also use some further assessments. If there is a significant slow down in growth post 1990s, could it have led to reduction in kinetic effect as proposed by Watson (1996) and Feng et al (2012), which suggest 10x increase in growth leads to -0.8‰ shift in calcite precipitated? This may be explored using speleothem LAB-S1, CRY-S1, both of which showed clear uptick in d18O values and their growth rates can be calculated readily with good accuracy.

Fifth, does the seasonal growth play a role in the observed d18O uptick? It is no secret that speleothems mostly grow in part of the year when there is CaCO3 over-saturated water and there is strong ventilation in the cave. Could changes in seasonality of rainfall and/or ventilation impact the interpretation of the data?

Lastly, how does fracture drip site and matrix site differ (or not) in calcite growth? If I am not mistaken, fracture/conduit path allows easier access to the rainfall, therefore they are more responsive to rainfall events. They also have shorter residence time in the vadose zone, which reduce the time they have to pick up Ca and DIC before reaching the cave. Therefore, it seems increased fracture flow path would lead to reduced calcite growth. If true, that could have some consequences: 1), fracture flow path may not be producing calcite/speleothem, and 2) if indeed there is a marked shift to more matrix flow path vs fracture flow path, growth rate of speleothem may show shift as well. Examine both speleothem growth rates and farmed calcite growth rate could help clarify this.

Reviewer #2 (Remarks to the Author):

I think there is a good paper embedded within the one submitted, but in my view the current understanding of the particular recharge processes at the cave sites in an unusually porous limestone in SW Australia doesn't seem to be complete enough to underpin the authors' attempt to give the study a wider significance and to warn that groundwater recharge in the whole region may no longer be reliable. I would like to see the stalagmite study published but in my opinion the paper should be extensively revised and the discussion confined to the recent effects of climate change as recorded in the composite stalagmite and drip-water record. Extrapolation to a wider area does not seem to be justified by the current state of knowledge of the links between recharge processes and isotope composition of drip waters.

My very long review is intended to be constructive and to assist the authors in reconsidering their paper in the light of these comments. Please see the attached file.

Summary and Evaluation

This paper presents interesting new data on the oxygen isotope composition of eight Recent speleothems from caves in southwest Australia and compares their compositions with those of drip waters and rainfall. The basic observations and data in the paper seem very robust. They build on on two decades of intensive monitoring in one or two of the caves, by a group who have long been leaders in cave monitoring and in relating drip hydrology to speleothem chemistry, and in interpreting the palaeo-records of speleothems as proxies for past climate changes.

The composite data on drip waters and recently deposited calcite show that d18O in both has risen by more than 1‰ over the last three decades. Both the amount of the rise and the values currently reached exceed any seen in the last 800 years in the stalagmite with the longest record reported here, although other long records have recently been reported elsewhere (see below). The paper attributes this unprecedented rise in d18O to changes in the amounts of recharge to the unsaturated zone of the porous calcarenite above the caves, along with a somewhat speculative interpretation of the pathways that are followed as rainwater enters and passes through the soil and percolates downwards through bedrock.

The paper goes on to make claims about the wider significance of the cave record for replenishment of regional groundwater in southwest Australia, where there is a strong dependence on it for all forms of water supply. This part of the argument rests on the similarity between the frequency distribution of d18O in a subset of cave drips and that of groundwater samples from the whole Perth Basin. The subset consists of drip sites that have relatively high and variable flow rates, and are thought to derive part of their flow via pathways through fractures. Their average d18O is lower than that of the slower, more constant drip sites that are believed to derive from flow through the porous rock matrix, although the frequency distributions of d18O of the two groups overlap. The paper attributes the upwards decadal trend in d18O to a decline in the component of recharge that follows the 'fracture' pathways. On the basis of the similar distributions of d18O between the fast drips and regional groundwaters, the paper claims that this shows that "rainfall recharge to groundwater across this region is no longer reliably occurring", and that "shallow karst aquifers [have become] disconnected from recharge due to regional drying". On the basis of a careful reading of the paper, the supporting materials and several previous publications by the same group I think these statements are at least disputable and are likely to be incorrect. After all, dripping and therefore recharge to the deeper unsaturated zone is manifestly continuing in the caves under study, despite a long-term reduction in rainfall and falling groundwater levels. The authors appear to be over-egging the pudding. A further claim is that the study shows that a global network monitoring d18O in cave drips together with analysis of recently deposited calcite could provide "early warning of reduced groundwater recharge elsewhere". This may be so, but would need to be demonstrated for the majority for karst aquifers which are generally in fractured, ancient limestones with low primary porosity, so have different flow characteristics from the Quaternary calcarenites that are the subject of the paper.

Notwithstanding the above criticisms, the paper does convincingly demonstrate that the isotopic composition of recharge as represented by cave drips has changed in recent decades and also shows that the water balance between evapotranspiration and rainfall has altered in rough parallel with this as rainfall has declined. The 800 year proxy record of drip water d18O provided by one of the stalagmites suggests that this may be unprecedented, a conclusion that agrees with an independently published proxy for century-scale trends in southwest Australian rainfall based on teleconnection with snow accumulation in Antarctica. This conclusion itself provides a warning of the seriousness of current climatic change for and would justify publication aimed at a wide readership in a journal such as Communications Earth & Environment. Unfortunately the interpretation of the data is rather difficult for a general readership to follow and the paper would benefit from careful rewriting with more explicit explanation of the conceptual models of recharge to the caves that have been previously published. These models underpin the arguments of the paper but are also sources of uncertainty in the interpretation. It would be best to acknowledge that, as well as drawing back from some of the wider claims and recommendations regarding the universality of the conclusions of this specific study.

Constructive (I hope) Discussion

The arguments and narrative of the paper rely on the reader knowing the literature produced previously by the group and being aware of the conceptual models of recharge processes developed therein. In order to understand the arguments being made I had to look up and study items 11, 26, 30 and 46 from the reference list, plus an additional item not cited in this paper (Mahmud etal., 2018. *Hydrology and Earth System Science*, **22**, 977-988). With the benefit of this background reading, I felt that some of the conclusions drawn in the paper were not fully substantiated by the data.

The fundamental problem I find is that the interpretations of recharge processes in the paper are based on a conceptual model, but the actual processes of recharge and deep percolation have not in fact been monitored. Consequently the conceptual model lacks detail and invokes mechanisms that have not been proven, such as a threshold effect in initiating fracture flow and the continuity of fracture pathways between the base of the soil and the cave. The previous studies make clear the fact that drip sites in one of the caves (Golgotha) have been mapped and monitored in great detail, and that rainfall and evapotranspiration have been measured or estimated in the immediate area, but there are no direct observations of the processes by which rainfall becomes recharge and feeds the drips that in turn precipitate the stalagmites. The Golgotha cave lies in an area of Eucalyptus forest with a dense understory, which probably inhibits direct evaporation from the soil by shading, producing a low ratio of evaporation to transpiration. This is important because while evaporation isotopically fractionates the residual water in the soil and produces higher values of d18O, transpiration does not. The soils are sandy and vary from 0.3 to 3 m thick. Thus some parts of the ground possess a deep store of soil moisture, but others do not and intense or prolonged rainfall may saturate these.

The rock in which the caves lie is a very porous calcarenite. Thus moisture can move downwards through the bedrock as unsaturated flow in a sandy porous medium. In the roof of Golgotha cave the rock displays some fracturing and the development of solution pipes, dissolved presumably by concentrated former flows of recharge water. In Ref.11 Lidar mapping of the cave roof is used to characterise drip points in terms of the presence or absence of a fracture, those without a fracture being fed by flow from the matrix of primary pores in the calcarenite. It is shown in that paper and in Mahmud et al (2018) that drips from fractures have greater variability in their discharge than those from the matrix. References 26 and 46 demonstrate that there are significant offsets of 0.6 – 1 ‰ in the average d18O values between these two types of drips.

A striking feature of the cave roof is that both stalactites and drips are concentrated in roughly circular damp areas of the roof, a few metres across, whereas the intervening areas are dry. This implies that vertical flow through the unsaturated calcarenite must take place in fingers with higher water content, separated by volumes of calcarenite with a much lower water content. As water cannot enter the cave void unless the pore water pressure is at least one atmosphere, only those parts of the cave roof that intersect these fingers are close to saturation and able to drip water into the void. (This applies to fractures as well as intergranular pores). Various factors may assist in concentrating downward flow of moisture into fingers, including bedding surfaces and clay-rich layers in the calcarenite, variations in the soil thickness, and the presence of fractures. The formation of fingers in sandy porous media is easily observed in both lab and field, wherever a wetting front separates a layer of high water content from underlying sand with a much lower water content. The phenomenon arises from the non-linear relationships between hydraulic conductivity, soil water tension (i.e. non-gravitational hydraulic potential), and moisture content in unsaturated sand. Reference 46 contains photographs of one macro-finger of high moisture content which is visible in the cliff wall of the doline outside the entrance of Golgotha cave.

It is important to note that the role of fractures in channelling water to drip points has only been established by observing their presence on the cave roofs. It is not known whether fractures form a linked network across the whole 25 – 40 m thickness of the unsaturated zone above Golgotha cave, or the other caves in this study. Nevertheless the paper under review uses terms such as 'fracture pathways' and 'focussed flow' without defining what they mean or explaining the evidence on which these concepts appear to rest. Reference 30 presents time-lapsed electrical resistivity imaging of the unsaturated zone above Golgotha cave in different seasons. This supports the conceptual model of fingering at macro-scale but isn't detailed enough to image either the cave itself or the meso-scale fingers that are evident on its roof. The electrical imaging does not provide any information on fracturing.

Based on the fact that the d18O of drip water shows seasonal cycles, Reference 46 suggests that the residence time between the surface and the cave roof is less than one year. Given that annual recharge very approximately amounts to about 700 mm depth of rainfall and the cave roof at Golgotha is about 30 m down, a one year transit time implies that the *average* volumetric moisture content of the unsaturated zone must be around (0.7 x 100)/30 = 2%. The porosity of the calcarenite is 30 – 50%. Therefore if the rock in fingers were almost saturated rock the fingers themselves would have to occupy less than one fifteenth to one twenty-fifth of the total rock volume, more if the moisture content of the fingered rock were lower than near-saturation, and/or if the transit time were longer than one year. Because the unsaturated permeability of sands increases non-linearly with moisture content, the overall proportion of fingered rock and its average moisture content will adjust to the flux of recharge being transmitted downwards. Thus, as recharge fluctuates seasonally, the extent and moisture content of fingering will also change. This appears to be broadly supported by the electrical resistivity measurements in Reference 30. If there are long term changes in the amount of recharge because of climate change, the degree of fingering and the moisture content within fingers may also change in the long term. It is important to recognise that all this may occur without needing to invoke the presence of connected fractures as rapid-transit pathways through the whole unsaturated zone, though this is not to deny their proven presence as local pathways at the cave roof.

A crucial element in the paper's argument is the observation that high-flow drips have a lower d18O than low-flow ones. The key step is the use of the conceptual model of recharge to account for this difference. Attribution of the low-flow drips to predominantly matrix flow is supported by general considerations of flow through porous media discussed above, with patches of near-saturated rock forming on the cave roof and allowing water to drip. The fact that there are few stalactites between these patches testifies to their stability over the long term. However the high-flow drips occur amongst the low flow ones so the question is, why is their d18O less than that of the low-flow drips which itself approximates average rainfall? The conceptual and numerical modelling in Ref 46 attributes this to a separate reservoir feeding these drips, but the nature and location of such a reservoir is difficult to envisage. In contrast, the present paper appears to envisage fractures as providing a continuous rapid transit pathway from the surface, but one that is only activated by relatively large or intense falls of rain. The reservoir effects of such pathways would be small compared with the fingers of unsaturated flow in which they must be embedded, and there would inevitably be some migration of water from the fracture into a matrix with lower capillary potential. Uncertainty about the lengths of individual fractures and the presence of a percolating network of fractures remains, however.

An important consideration is whether the isotopic difference between high and low flow drip sets has persisted as the general rise in d18O has occurred. This is not apparent from the data in this paper, but Fig 5 in Ref 26 indicates that it has, with individual members of both groups showing much the same upwards trend. This suggests that the mechanism producing the difference has persisted even as the frequency of near-saturated conditions in soils has declined, and with it the supposed frequency of recharge by large or intense falls of rain with lower than average d18O. This is difficult to reconcile conceptually with the fact that the mean d18O of rainfall has not changed significantly during the period of records. Without details of the relationships of rainfall amounts to d18O, it is difficult to evaluate whether this persistence is compatible with the hypothesis that the lower d18O values in high-flow drips derive from large or intense rainfalls that exceed a threshold for initiation of fracture flow.

A mechanism that might account for the rise in d18O in both groups of drips is isotopic enrichment of soil water by evaporation. Although the ratio of evaporation to transpiration is likely to be in favour of the latter, it may have changed in the direction of relatively greater evaporation as soils became drier and trees limited their transpiration during longer periods of moisture stress. The residual moisture in the soil at the end of summer is likely to be quite highly fractionated and this would then be mixed with relatively unfractionated water from the succeeding wet season's rainfall. As the threshold for supposed fracture flow is more likely to be exceeded in winter, the O-18 enriched soil water from the previous summer may remain a significant component of the mixture when recharge into fractures occurs. This is certainly very speculative but the idea is testable by comparing d18O and d2H in drip waters and looking for indications of evaporation, especially among samples whose d18O values are relatively high for each site. Inspection of Fig.4 in Ref 26 does not provide support for the idea, although it is noticeable that the fast drips 2B and 2E have lower d18O and higher deuterium excess than the remaining three which have compositions that lie close to the mean values of rainfall. This pattern has been observed elsewhere (e.g. in Grotte de Villars, France) and may be due to additions of condensate from vapour derived by evaporation from soil moisture. In principle such additions are possible in the summer period when the cooler denser air in the porous bedrock will drain into the cave (incidentally raising the cave air CO2 levels) while near the surface air is drawn downwards through warm sandy soil into cooler regions at depth. Because connected fracture paths would transmit much larger local fluxes of air than the matrix, this mechanism might enrich the water in them in O-18.

It is clear from this lengthy discussion that in this reader's mind at least there is much uncertainty regarding the possible reasons for the disparity in d18O values between high and low flow drips. I am not convinced that it is a simple matter of a bias towards isotopically light recharge. In view of this uncertainty, is it wise to promulgate the opinion that the rise in d18O observed in the record, and in fast drips as well as slow ones, is merely due to a reduction in the contribution made by a still-hypothetical fissure flow? The rise in d18O is an established fact but the mechanism by which it occurs, and its relationship to recharge amount remains too uncertain for the next step in the paper, the warning that “fracture” or “focussed” flow is in danger of ceasing altogether.

One other comment occurs to me having looked at Fig.4 in Ref 26. This shows the records from all four Golgotha speleothems, comprising half of the group of eight discussed in the present paper. All four have records extending back to 1200 CE, yet only one – GL-S4 – is mentioned in the present paper and compared with the Law Dome ice record. The other three fit this record less well. Moreover, three of the four have d18O values between 1150 and 1400 CE that exceed all values in the respective stalagmites since then, and even exceed those of the drip waters and farmed calcite in the present century. Two of the three have high values at the start of their growth, one being GL-S4, while the third (GL-S3) has the highest d18O of all just prior to a cessation of growth that lasted almost six centuries. Such patterns of high d18O at start and end of growth of speleothems are common world-wide and are generally correlated with similar changes in d13C of the calcite. This is an indication that the amount of fractionation by degassing is affecting both isotopic systems. Among other factors, greater degassing is promoted by longer residence times between drips. Could this also be a factor affecting the farmed calcite as well as the speleothems in the present study? However, it would not produce the observed rise in the d18O of the drip waters unless there were significant amounts of evaporation at the drip site, and this does not show up in the cross-plot of d18O versus d2H in Ref 46.

Detailed comments on the typescript

Line 1. Title: This is misleading. There is abundant local evidence of decreasing recharge from independent sources (weather records and calculated water balance, declining water table in two caves, as in Fig.3d, So the speleothem warning is not ‘early’, though it is timely. As discussed above, the mechanism by which 18O of drip water and calcite might act as a proxy for recharge amount has not been fully established. In addition, the d18O values seen in the last few decades were exceeded in the period 1150-1400 CE in some of the speleothems. I suggest a different title, such as “Speleothem data suggest that current drying trends in Southwest Australia are unprecedented in the last 600 years”. This is a more accurate indication of what can actually be shown from the data by rigorous argument rather than loose analogy.

Line 10. “shallow karst aquifers becoming disconnected from recharge”. It’s not clear what this means in hydrological terms. It suggests that dripping ceased or was supplied from some other source than rainfall.

Lines 11-12. “recharge no longer reliably occurring”. The paper shows that there has been no cessation of dripping. So recharge is still occurring and has not yet been interrupted.

Line 12-13. I think this is the main point of the paper.

Lines 13-16. This might be a good idea for drip water monitoring but in the context of this paper it looks (to me) like window-dressing.

Lines 16-18. In what way has the *causal* connection between rising d18O and water balance or recharge amount been proven? I think there is still an undistributed ‘grey box’ between the two, so the validation rests mainly on visual correlation with the rainfall record over the last few decades. It would perhaps be useful to explore that more fully.

Line 25. Some further references on the sensitivity of groundwater recharge to climate change:

Yusoff, I., Hiscock, KM. & Conway, D., 2002. Simulation of the impacts of climate change on groundwater resources in eastern England. *In* Hiscock, KM., Davison, RM. & Rivett, MO. (eds.) *Sustainable Groundwater Development*. Geological Society of London Special Publication 193, 325-344.

Herrera-Pantoja, M and Hiscock, KM, 2008. The effects of climate change on potential groundwater recharge in Great Britain. HYDROLOGICAL PROCESSES 22 (1), pp.73-86.

Herrera-Pantoja, M., Hiscock, KM. & Boar, RR., 2011. The potential impact of climate change on groundwater-fed wetlands in eastern England. Ecohydrology, **5** (4), 401-413.

Line 31. 'Soil water balance' would be a better term to use than "rainfall" as it is the balance of rainfall, evapotranspiration and their effect on the occurrence of the state of field capacity that are critical for production of recharge.

Line 36. In the context of the paper it is important to emphasize that the high permeability is primary, i.e. intergranular. To avoid confusion I suggest removing the word "diffuse" as it adds little to "porous" and in the karst literature the term 'diffuse groundwater flow' refers to non-conduit flow, typically in fracture networks. So I suggest, '…can be porous flow where the host rock has high primary permeability."

Line 37. "two flow types" Four types of *pathway* have been mentioned. Are the flow types distinct in these? Unsaturated water flow under a gravitational gradient in fractures, fracture zones and conduits, versus unsaturated flow in a porous medium under the combined gradient of matrix potential and gravity. It looks as if it is the potential gradient that differs, rather than the type of flow.

Lines 40-41. Speleothems provide evidence of the *occurrence* of recharge, but give only a relative indication (or none) of its *amount*.

Lines 44-46. Good point which could be placed at Line 35 for greater impact.

Line 47. The following paragraph jumps rather suddenly to SW Australia.

Lines 71. Define what is meant by "focussed flow paths". In the context of the calcarenite aquifer these could be fingered flow in the porous medium, and/or flow along sedimentary discontinuities, as well as flow in fractures. Only the first of these has the effect of 'focussing' the flow – in the sense of convergent flow from a large cross-section into a much smaller one. 'preferential flow' is a widely used term that might be better.

Line 72-73. What is demonstrated in the paper is that the uptick in d18O is unprecedented. The causal link to recharge reduction is rather less certain.

Lines 75-76. Is this a universal effect? The wider applicability of this study to other systems depends on demonstrating conclusively that it is. (I feel that the mechanism isn't fully understood even in the system under study here.)

Lines 77-78. The last part of this sentence did not make sense on first reading, but on returning to it after reading the whole paper several times I think it should be omitted. Although it is incompletely expressed, the argument of the paper uses drip discharge patterns to attribute drip sites to one of two categories, low and high flow sites. Those with low, constant flow are attributed *a priori* to flow in the matrix whereas those with high and variable flow are regarded as being fed by fractures. The paper goes on to attribute the uptick in d18O to a decline in the importance of the fracture category, This attribution does not validate the identification of the original category with fractures, and to claim it does is a circular argument. (Only the drip characteristics have actually been observed, the attribution is an inference.)

Line 85-86. The latest rise in MND-S1 starts in the early 1970s, not the 1960s when d18O is falling slightly. The values reached at the end of this record in the early 1990s are lower than those in the 1940s. This last point is also true for LAB-S1 and YD-S2.

Line 88. “composite data” – does this refer to the composite record in Fig.3a? As it is an average it represents the common signal but does not prove commonality. Perhaps the phrase ‘comparison of records” (as in Fig.2) would be better?

Lines 89-91. The drip water data have been converted to equivalent values for precipitated calcite, using a fractionation factor based on comparing drip waters and farmed calcite. This factor is empirical and not the equilibrium one appropriate to the cave temperature. Some explanation of all this is needed, otherwise the comparison makes no sense because the media and isotope scales would normally be different.

Lines 93-95. The two references (23, 24) would be better placed after “multiple processes”. Neither of them is specifically about category iv in the list.

Line 99-100. “recent warming[16]”: does this refer to temperatures on the surface of in the cave? One would expect a lag in the cave temperature that might be several decades behind the changes in external temperature. Therefore 0.27 ‰ is probably an upper bound to the effect of cave warming. (Ref 16 appears to be mainly about rainfall; does it also detail local changes in surface temperature?)

Line 101-103. This statement is misleading. The empirical fractionation factor between drip water and farmed calcite is stated at Line 247 to be 1.0318 (Ref 26). The equilibrium value at cave temperature (14.6 ^{o}C) is 1.0307 and the discrepancy clearly indicates isotopic disequilibrium. In terms of temperature, the Golgotha calcites would indicate a temperature of 9.8 ^{o}C if assumed to have been deposited at equilibrium as defined by the Kim & O’Neil equation. Though there have been suggestions that Kim & O’Neil’s experimental data may not reflect ‘true’ equilibrium, they have recently been replicated in a laboratory simulation of stalagmite growth (EL-Shenawy, MI; Kim, ST and Schwarcz, HP, 2020. Carbon and oxygen isotope systematics in cave environments: Lessons from an artificial cave "McMaster Cave''. *Geochimica et Cosmochimica Acta* **272** ,137-159). This showed that slow degassing from a pool resulted in equilibrium deposition in full agreement with the original 1997 Kim and O’Neil study, whereas deposition from a thin water film fed by dripping onto a ‘stalagmite’ showed disequilibrium and larger empirical fractionation factors. This last appears to be the case for Golgotha cave too. Other recent lab studies confirm this (Hansen, M et al., 2019. Chemical Geology **509**, 20-44 and citations therein).

Lines 106-7. There are other possibilities, for example the rise in d18O may be driven by a greater proportion of rainfall being evaporated as opposed to transpired, as rainfall has declined and longer periods of moisture stress may have limited transpiration. See “Constructive Discussion” above.

Lines 123-124. This is the crucial observation made in the paper. (I agree.)

Lines 129-130. This effect was first noted by JC Vogel and H Van Urk, 1977. Isotopic composition of groundwater in semi-arid regions of southern Africa. *Journal of Hydrology*, **25**, 23-36. The paper is still being cited.

Lines 132-135. The statement is true but the argument is not conclusive because all of the three rainfall distributions overlap with both drip types (Fig.4).

Lines 135-136. Refs 14 and 26 are based in part on data from the same SW Australian caves as in this study. Is there a danger of circular argument in this sentence?

Lines 137-138. "… supporting that rainfall recharge to groundwater beneath these caves *occurs primarily via fracture flow*." My italics – why? Are all the other aquifers fractured? This argument should be made in more detail, especially as the continuity of the fracture network in the calcarenite aquifer is unproven.

Line 143. "… the only mechanism that can explain the magnitude of the d18O uptick…". The difference in d18O between drip types is not large enough for this. If diminished fracture flow really is the explanation, that would imply that the d18O of the fracture waters must be even lower than the values seen in the high-flow drips, i.e. the latter must be mixtures of fracture and porous pathway waters. That in turn implies greater selectivity of low-d18O events during recharge, but could also explain why the fast-drip d18O has continued to rise. However this explanation would also imply that the slow drips are mixtures too, with the proportion of fracture water declining over time.

Line 146 refers to three caves with water table measurements but only two are shown in Fig.3d. Why not show the third?

Lines 149-151. I think that this data is neglected in the paper. Is the aquifer at Jewel Cave affected by abstraction? If not, the long term decline is pretty clear evidence that recharge has been less than outflow since about 1980. Because of the difficulty in establishing a convincing causal connection between d18O and amount of recharge, this is better evidence that the latter has declined than the stalagmite record, at least for the last few decades.

Line 168. Might "preferential' be a better word than "focussed"?

Line 176. 'disconnection' is an imprecise expression which the use of quote marks seems to acknowledge. The high-flow drips are still flowing so the preferential flow has not ceased, therefore there is no 'disconnection' from rainfall recharge. This needs much more careful phrasing, especially as the paper goes on to extend the idea to all the aquifers of the Perth Basin, purely on the basis of their similarity in d18O distributions with high-flow drips.

Line 180. "The replication of the uptick across the southwest Australian region…" This statement is tendentious; only one cave out of eight is outside the peninsula in the southwest. Its record is one of those in which the d18O values at the end of the stalagmite record are exceeded by earlier peaks. This record (YD-S2) is certainly compatible with the others but its uptick since 1985 must be set against four earlier such rises in the same stalagmite since 1915. This pattern does not replicate what is interpreted in this paper as an event that was unprecedented in the past 800 years. It is a very weak basis on which to claim in Lines 181-182 that "no other mechanism could produce this common response across multiple caves".

Line 183. "… a robustly-constrained process-based relationship…" I do not agree that the process basis for the rising d18O trend is fully understood and therefore I don't think it should be claimed to be robust. The language here is rhetorical rather than factual.

Lines 197-200. Comparison with the Law Dome record. Fig.5 shows the actual record from Law Dome, It appears to be negatively correlated with the speleothem record. One has to go to the references to discover that rainfall in SW Australia is also negatively correlated with ice accumulation in Antarctica. Either this should be explained, or Fig.5 should show reconstructed values for SW Australia, to agree with the text here.

Line 201. What does "we confirm" mean? It is not true that there are no uptick events in the last 800 years. In Ref 26 Fig.4 GL-S3 shows a very strong one between 1260 and 1360 CE, but it is not "replicated" by two other stalagmites in the same cave, one of which is the only one presented here. This omission could be regarded as misleading. It certainly weakens the statement that the recent uptick is unprecedented.

Lines 202-203. "… current decoupling of rainfall to groundwater recharge…" The data do not support decoupling; dripping continues. It is the d18O uptick which is unprecedented (since 1400 CE).

Lines 208-209. I suggest insertion of 'the d18O of drip waters and calcite deposited from them in' before "caves", and 'as being' at the end of Line 208. In line 209, "early warning" should be replaced by 'an indication'.

Lines 210-211. I strongly recommend that this statement be omitted altogether. The data and its interpretation do not support it.

Line 212. Change 800 years to 600 and include the long record shown in Fig.4 of Ref 26.

Line 217 to 220. This recommendation would only be a useful one if and only if (a) the correct reasons for d18O decline have been identified in this study (which I think is still in doubt), and (b) if most karst aquifers shared the same type of flow in the unsaturated zone as the calcarenites in SW Australaia, which they do not. I think that the whole sentence is best omitted.

Line 246-247. See comment on Lines 101-103.

Line 324. Reference is incomplete.

Figure 1. The use of colour shading and contours on the main map replicates information and makes the map harder to read at a glance. Perhaps better to remove the colour shading and the part of the key that refers to it?

Figure 2:

The farmed calcite and dripwater continue the upward trend seen in the stalagmites but in individual cases (GL-S1, GL-S3 and LAB-S1) there is near enough continuity between the two types of records to see definite 'jumps' of 0.5, 0.35 and 0.2 ‰ respectively. Could this be evidence that the fractionation between drip water and speleothem is greater than that between drip water and farmed calcite ? The film thickness may not be the same on both types of surface. Correction for this would not remove the uptick but it would reduce its amplitude somewhat.

Only five of the eight speleothem records show an uptick that goes outside the range of previous values in the same specimen.

Lines 504-506. This seems to imply that the normalisation of the d18O series was to 20th century warming values. That isn't correct is it?

Figure 3c. The yellow and orange colours are difficult to distinguish.

Figure 4. The paper does not discuss the criteria used to group drips into "fracture" and "matrix" categories. This should be explained.

Line 530. A reference to an explanation of the kernel density method is needed.

Lines 532-534. This suggests that the histograms derive from just two drip sites in the same cave. Is this an adequate basis for extrapolation to the whole Perth basin? I think not.

Figure 5. (a) this panel compares speleothem and farmed calcite from different drip sites. Given my comments on Fig.2, is this an honest representation? Would it not be better to show the composite record instead of the farmed calcite data?

(b) This replicates Fig.4. Could the two Figures be combined?

(c) This does not show reconstructed rainfall. Should the scale be changed to do this?

Supplementary Material

Supplementary Note

Lines 2-3. This information should be in the main text, with an indication of the fact that as a host rock, very porous calcarenites are untypical for cavernous aquifers which tend to be in low-porosity, well-cemented limestones with fractures dominating their hydraulic conductivity.

Lines 12-13. This information should also be mentioned and discussed in the main text. Fractionation of soil moisture has not really been discussed at all and could be a contributing factor to the d18O increase.

Supplementary Table 1. The information on vegetation and on soil thickness and its variability over each site should be mentioned in the main text.

Supplementary Figure 4. This has three panels but the caption refers to a fourth and does not match the Figure. Once again the Law Dome record is said to be a record of reconstructed rainfall in SW Australia. It is not; the scale should be changed if it is to be treated as such.

We thank the two reviewers for their time and thoughtful comments that have helped us to improve the manuscript. In particular, we thank Reviewer #2 for their 'Constructive Discussion' which very much helped us clarify our terminology and encouraged us to provide an improved evidence base for our interpretations. This has resulted in a lengthy response to reviewers, reflecting the thorough review of #2 and our considered responses. We largely agree with all the reviewer comments, and where we do disagree, we have provided our reasoning and sought to provide a revision that we hope will be a suitable compromise. In summary, in our response we have:

- Improved and clarified our terminology and definitions of flow paths and added more detail to the sites to support the hydrological interpretation of our stalagmite data. This has resulted in added text in the main text and a new supplemental text section.
- To further demonstrate the relationship between preferential flow and recharge to the groundwater below the caves, we have added a new figure with cave drip data that shows increased flow along preferential flow pathways and recharge to the local water table in response to higher rainfall.
- Reviewer #2 provides a thought exercise for all other potential processes that could theoretically have generated the $\delta^{18}O$ uptick in the data. Evidence for why we do not consider these processes to be occurring is presented, which we believe has resulted in a more robust support of a flow path control. To further provide evidence for this, we include a new figure of the local meteoric water line showing that evaporation has not significantly modified the drip water $\delta^{18}O$ values from the local rainfall $\delta^{18}O$ values.

A track changes version of the revised manuscript is attached and our detailed point-by-point response to reviewers is below (blue text). We have taken the opportunity to make cosmetic changes to the text to improve the manuscript and changed the colour palette of the figures to be colourblind friendly.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of Manuscript COMMSENV-22-0366-T entitled "Caves provide early warning of unprecedented decrease in rainfall recharge of groundwater".

In the manuscript, the authors presented new and previously published d18O data of speleothem from five caves along southwestern Australia coast. Authors combined speleothem records with farmed calcite, drip water d18O data from one of the caves, and presented a composite speleothem record for 1910- 2020, which showed ~1‰ uptick in d18O values since 1990s. Authors argued that this positive change is due to the reduced fracture flow (lower d18O values due to amount effect) vs matrix flow (higher d18O values), as a result of reduced rainfall recharge. Authors further stated that this change in speleothem d18O values indicates reduced recharge to groundwater from the rainfall in the region.

Considering the numerous speleothems within each individual cave, it is not an easy task to acquire multiple speleothem samples from different caves across a large region that overlap a common growth period. They carry important "big picture view" of the regional climate and improve our understanding of regional and global climate forcing. As such, this study is important, creative, and warrants the publication.

There are some significant issues, however, I would like authors to address. They are summarized below.

We thank Reviewer #1 for their positive comments and provide detailed responses below. Please note that we have labelled our responses as (A, B, etc.) so that we may refer back to them.

First, a minor problem is that it would seem there is no age constrain on Golgotha speleothems for the duration presented in the study (1910-2020). Treble et al (2022), ref 26, showed that dated layers for these speleothems from Golgotha are all older than 1910. Considering milling increment of 100-200μm and very short total milling length of these samples in Fig. S1 (<1cm), each sample's age would have some significant uncertainties. It would be informative to add these error bars onto Fig. 2.

(A) We refer Reviewer #1 to the first paragraph of the Methods section "Speleothem chronologies" where the chronology method for each speleothem is given. It was stated (Golgotha speleothems) "GL-S3 and GL-S4 were determined from annual laminae counting" and "GL-S1 and GL-S2 chronologies were determined from 'bomb pulse' detection using radiocarbon analyses". These are the state-of-the-art methods for providing age constraints for the studied interval (1910-2020) and it is thus incorrect that the age constraints for these materials are all older than 1910.

Regarding age model uncertainty, we refer Reviewer #1 to the Methods section "Composite speleothem record" where it states "We considered uncertainty in the age-depth models but this is least at the youngest end. i.e., 0% at the year of collection and still small at 1900 CE (range ±3 to 12%)". We take the point that it is not intuitive to look for this information in the Methods section so we have added the following sentence to Fig. 2 caption: "Chronologies for all speleothems were published previously (see Methods)." We note also that these age-depth models and their uncertainties have now been supplied to SISAL and we updated the Data and Code Availability Statement accordingly. In doing so, we noticed that the range of uncertainty should read "range ±2 to 18%, 2σ" and have corrected this on the revised manuscript (Line 560).

Second, regarding the observation of d18O uptick since 1990s. This doesn't seem to be prominent in sample GL-S1, YD-S2, GL-S2, MND-S1. Furthermore, if farmed calcite and dripwater derived calcite d18O is used as part of the record to produce this "uptick", they are only from one cave. It is also risky to combine farmed modern calcite with speleothem calcite for a composite record, as there are plenty unknowns regarding potential differences between these two, the risk goes even higher when we derive calcite d18O values from drip water d18O values using a fractionation factor that may not have been the same during the growth of the speleothems.

(B) GL-S1, GL-S2 and MND-S1 all show a positive trend in $\delta^{18}O$ values by the mid-1980s. For GL-S1 and GL-S2, this trend continues in the subsequent measurements of drip water $\delta^{18}O$. We confirm that the drip water measurements are all from one cave. Regarding the comment on the robustness of the farmed calcite $\delta^{18}O$ values and their fractionation factors, this was discussed extensively in ref. [1] which was cited in the text. As in ref. [1], we emphasise that the most objective assessment of enhanced isotopic disequilibrium and kinetics are the calculation of fractionation factors. We have added additional text (Line 160-168) to make this point clearer: "This was constrained numerically by demonstrating that the O isotopic fractionation factors ($\alpha_{calcite\text{-}water}$) were spatially consistent across the four sites and temporally consistent over the period of rising drip water $\delta^{18}O$ values (see ref. [1] and their Table 2 and their Supplementary Note 2)". And Line 136-138 "These three records replicate the rising isotopic trend recorded in the drip water monitoring during the respective periods of overlap with the monitoring data (Fig. 2), confirming the robustness of the uptick as a regional signal (Fig. 3a)". We note here that the bulk fractionation factor, i.e., the average $\alpha_{calcite\text{-}water}$ value, was applied to convert the drip water values as described in the Methods.

Regarding YD-S2, it has been shown in a recently published paper by ref. [2] that this record has been affected by fire. As it has been shown that this can complicate the $\delta^{18}O$ record we have decided to remove this record from the paper.

Thirdly, with the d18O uptick established, the difference in d18O values between diffuse/matrix and fracture/conduit flow path seems an inadequate explanation for the 1‰ uptick. Supp table 4 in ref 26 describes a difference as small as 0.26‰, with the largest difference from sept-13 being only 0.67‰ when uncertainty is included. This contrasts with uptick of 1‰+ shown in Fig. 2.

(C) We note that reviewer must be referring to Supp Data 4 in see ref. [1] rather than Supp Table 4. Regarding the observed difference between matrix and preferential flow of 0.67 ‰ being too small to explain the 1+ ‰ uptick, please see point in Line 294-297 highlighting that we did not intend for the two flow-types to be interpreted as pure endmembers, i.e., all drips measured are a mixture of diffuse versus preferential flow. Additionally, we have expanded our justification for the reduction in recharge by preferential flow as being the driver of the uptick, including consideration of additional processes (Line 225-273). Please see our response F below.

(D) We do apologise for any confusion that might have arisen from the definitions of the flowpaths given in the original manuscript. Reading the above, as well as other comments by both reviewers it seems likely that the definitions supplied led to the flowpaths being envisaged as "pure" matrix versus "pure" preferential flow, whereas the monitored drips presented are supplied by a mixture of water from diffuse-matrix as well as contributions from preferential flow from fractures/conduits. It is the contribution of preferential flow that gives rise to the difference in mean $\delta^{18}O$ values between drip sites[1], and we interpret in this manuscript that it is a reduction in this contribution over recent decades that has cause the 'uptick' in the drip and speleothem $\delta^{18}O$ values. For clarity, we reproduce the definitions of the flow paths given in ref. [3] and in ref. [1] in this manuscript (Line 100-102) and clarified the terminology throughout e.g., preferential-influenced, diffuse-dominated. We hope that this clarification resolves the above query and that it also accounts for several of the other questions raised by Reviewer #1 and many of the questions asked by Reviewer #2.

Fourth, the impact of evaporation of water along matrix flow path, and kinetic effect on calcite d18O values were not adequately addressed. They could be assessed using additional data that may already exist.

(E) We thank Reviewer #1 for their feedback on this. We've addressed the specific concerns point by point below.

If matrix flow water is 1‰ more positive than ground water/fracture flow, could it be due to prolonged residence time of these slow-moving water in vadose zone and their susceptibility to evaporation before reaching the caves? If so, could marked reduction in precipitation in the region post 1990s have led to more positive matrix water d18O values that contributes to uptick in calcite d18O? Additional dD vs d18O plot of rainfall, matrix drip water, along with GMWL and LMWL could help clarify this.

(F) We thank Reviewer #1 for raising this point and the opportunity to further clarify this process here and in the manuscript. We agree that the process of evaporation could raise the $\delta^{18}O$ value of water and thus it is appropriate to further explore this. We believe that there are two points to be discussed here: 1. Could evaporation be responsible for the difference in mean values between drip sites? and 2. Could this process have become more important over recent decades to produce the uptick in drip water $\delta^{18}O$ values?

Regarding point #1, we consider evaporation in the soil first and then consider evaporation lower in the karst profile (see Line 225-273). Rainfall is highly seasonal and occurs during the cooler months (April to October). Our soil moisture data[4,5] shows that soils become completely dry during the warmer months, due to high ET rates, such that when the next rainfall season begins, it can be assumed that there is little to no contribution of isotopically-modified soil water and any contribution would be greatly diluted by the newly infiltrated water. This is supported by a comparison of water isotopes from rainfall and drip water which shows that drip water has a similar d^2H vs. $\delta^{18}O$ slope as the local meteoric water line (LMWL) whereas waters affected by evaporation in the soil zone, i.e., under low humidity, would have slopes that are lower than the LMWL (see new Fig. 4). However, we would like to point out that the same diagnostic is not definitive if evaporation of the drip source water proceeds under high humidity. Under high humidity, isotopic fractionation of $^{18}O/^{16}O$ is limited to diffusion, and thus evaporation proceeding under high humidity (i.e, along deep flowpaths) will result in a similar slope as the LMWL (see Cuthbert et al., 2014). We have noted that the diffuse flow drips have a slightly lower slope than the preferential flow drips and that some evaporation of the slower moving diffuse flow drips may be occurring but estimate that this would produce a 0.1 ‰ offset between the flow types (see Line 266-269).

Regarding point #2, this implies that the relative importance of the hypothetical evaporation has become more important over time. High relative humidity has been monitored in Golgotha Cave since 2017 at 15 min intervals. Thus, for evaporation to be increasing over time, it would have to be driven by an increase in temperature or a consequence of a reduction in the net flux of water through the cave system which has not occurred. Cave temperature has been monitored at the same site in Golgotha Cave in discontinuous intervals since 2005 and has not risen (see new Supplementary Table 2). Observed relative humidity is also high (mean value 99%, typically >98%, range 95-100%) and with no trend over time since monitoring of relative humidity began in 2017. We acknowledge that less frequent recharge of the flowpaths by preferential flow over time would increase the potential for evaporation of slow moving water through the karst and this is consistent with our interpretation in the manuscript that there has been a reduction in the preferential flow component owing to the decline in rainfall.

To address both points, we have added text at L169-170, 225-273: including a new figure (Figure 4).

Kinetic fractionation due to variable growth rate (in mm/yr) of speleothems could also use some further assessments. If there is a significant slow down in growth post 1990s, could it have led to reduction in kinetic effect as proposed by Watson (1996) and Feng et al (2012), which suggest 10x increase in growth leads to -0.8‰ shift in calcite precipitated? This may be explored using speleothem LAB-S1, CRY-S1, both of which showed clear uptick in d18O values and their growth rates can be calculated readily with good accuracy.

(G) We thank the reviewer for raising this, however we can confirm that we do not observe changes in growth on the order of 10x in any of our speleothem samples (over any time scales) as was observed in the very high calcite deposition rates in ref. [6]. We also estimate that the calcite depositional rates in our study were 10^2 to 10^4 times lower than the ref. [6] study. For completeness, we have followed Reviewer #1's suggestion and added new text at Line 170-181, 537-538. These calculations may be reproduced using the laminae data archived with SISAL.

Fifth, does the seasonal growth play a role in the observed d18O uptick? It is no secret that speleothems mostly grow in part of the year when there is CaCO3 over-saturated water and there is strong ventilation in the cave. Could changes in seasonality of rainfall and/or ventilation impact the interpretation of the data?

(H) There has been no change in the seasonality of rainfall at any of the cave sites. Ref. [7] demonstrated that ventilation of Golgotha Cave is triggered seasonally when the outside air temperature falls below the internal cave temperature. Warming over recent decades could serve to shorten the ventilation season temporarily but this would not result in a trend in the duration of the ventilation season as the cave temperature would re-equilibrate with the average external temperature.

Lastly, how does fracture drip site and matrix site differ (or not) in calcite growth? If I am not mistaken, fracture/conduit path allows easier access to the rainfall, therefore they are more responsive to rainfall events. They also have shorter residence time in the vadose zone, which reduce the time they have to pick up Ca and DIC before reaching the cave. Therefore, it seems increased fracture flow path would lead to reduced calcite growth. If true, that could have some consequences: 1), fracture flow path may not be producing calcite/speleothem, and 2) if indeed there is a marked shift to more matrix flow path vs fracture flow path, growth rate of speleothem may show shift as well. Examine both speleothem growth rates and farmed calcite growth rate could help clarify this.

(I) We thank Reviewer #1 for the thoughtful points. Please also see our response D above, clarifying that the drip sites are mixtures of diffuse-matrix versus preferential-fracture rather than "pure" end-members of these. Please also see response G regarding speleothem extension rate. Deposition rates on farmed calcites were not quantified. We acknowledge that it is possible that activation of fracture flow could impact the saturation index (with respect to calcite; SIcc) data. Published SIcc values in ref. [7] that shows SIcc values for matrix (diffuse)-dominated sites were lower versus fracture (preferential) influenced flowpaths (e.g., site 1A versus 2E in ref. [7]. This also makes sense considering the longer transit time of water through the matrix.

Reviewer #2 (Remarks to the Author):

I think there is a good paper embedded within the one submitted, but in my view the current understanding of the particular recharge processes at the cave sites in an unusually porous limestone in SW Australia doesn't seem to be complete enough to underpin the authors' attempt to give the study a wider significance and to warn that groundwater recharge in the whole region may no longer be reliable. I would like to see the stalagmite study published but in my opinion the paper should be extensively revised and the discussion confined to the recent effects of climate change as recorded in the composite stalagmite and drip-water record. Extrapolation to a wider area does not seem to be justified by the current state of knowledge of the links between recharge processes and isotope composition of drip waters.

My very long review is intended to be constructive and to assist the authors in reconsidering their paper in the light of these comments. Please see the attached file.

We thank Reviewer #2 for the feedback put into this review. We do believe that the definitions of the flowpaths given in the original manuscript may have contributed to confusion (please see response D to Reviewer #1) and we further clarify these processes in the detailed response below. We believe this has strengthened the evidence for the relationship between reduced rainfall recharge and the $\delta^{18}O$ uptick. To further justify the discussion that the recharge decline is likely affecting the wider area we have now also included the regional rainfall decline in Figure 1.

We have provided point-by-point responses to the comments below.

Summary and Evaluation

This paper presents interesting new data on the oxygen isotope composition of eight Recent speleothems from caves in southwest Australia and compares their compositions with those of drip waters and rainfall. The basic observations and data in the paper seem very robust. They build on on two decades of intensive monitoring in one or two of the caves, by a group who have long been leaders in cave monitoring and in relating drip hydrology to speleothem chemistry, and in interpreting the palaeo-records of speleothems as proxies for past climate changes.

The composite data on drip waters and recently deposited calcite show that d18O in both has risen by more than 1‰ over the last three decades. Both the amount of the rise and the values currently reached exceed any seen in the last 800 years in the stalagmite with the longest record reported here, although other long records have recently been reported elsewhere (see below). The paper attributes this unprecedented rise in d18O to changes in the amounts of recharge to the unsaturated zone of the porous calcarenite above the caves, along with a somewhat speculative interpretation of the pathways that are followed as rainwater enters and passes through the soil and percolates downwards through bedrock.

The paper goes on to make claims about the wider significance of the cave record for replenishment of regional groundwater in southwest Australia, where there is a strong dependence on it for all forms of water supply. This part of the argument rests on the similarity between the frequency distribution of d18O in a subset of cave drips and that of groundwater samples from the whole Perth Basin. The subset consists of drip sites that have relatively high and variable flow rates, and are thought to derive part of their flow via pathways through fractures. Their average d18O is lower than that of the slower, more constant drip sites that are believed to derive from flow through the porous rock matrix, although the frequency distributions of d18O of the two groups overlap. The paper attributes the upwards decadal trend in d18O to a decline in the component of recharge that follows the ‘fracture’ pathways. On the basis of the similar distributions of d18O between the fast drips and regional groundwaters, the paper claims that this shows that “rainfall recharge to groundwater across this region is no longer reliably occurring”, and that “shallow karst aquifers [have become] disconnected from recharge due to regional drying”. On the basis of a careful reading of the paper, the supporting materials and several previous publications by the same group I think these statements are at least disputable and are likely to be incorrect. After all, dripping and therefore recharge to the deeper unsaturated zone is manifestly continuing in the caves under study, despite a long-term reduction in rainfall and falling groundwater levels. The authors appear to be over-egging the pudding. A further claim is that the study shows that a global network monitoring d18O in cave drips together with analysis of recently deposited calcite could provide “early warning of reduced groundwater recharge elsewhere”. This may be so, but would need to be demonstrated for the majority for karst aquifers which are generally in fractured, ancient limestones with low primary porosity, so have different flow characteristics from the Quaternary calcarenites that are the subject of the paper.

Notwithstanding the above criticisms, the paper does convincingly demonstrate that the isotopic composition of recharge as represented by cave drips has changed in recent decades and also shows that the water balance between evapotranspiration and rainfall has altered in rough parallel with this as rainfall has declined. The 800 year proxy record of drip water d18O provided by one of the stalagmites suggests that this may be unprecedented, a conclusion that agrees with an independently published proxy for century-scale trends in southwest Australian rainfall based on teleconnection with snow accumulation in Antarctica. This conclusion itself provides a warning of the seriousness of current climatic change for and would justify publication aimed at a wide readership in a journal such as Communications Earth & Environment. Unfortunately the interpretation of the data is rather difficult for a general readership to follow and the paper would benefit from careful rewriting with more explicit explanation of the conceptual models of recharge to the caves that have been previously published. These models underpin the arguments of the paper but are also sources of uncertainty in the interpretation. It would be best to acknowledge that, as well as drawing back from some of the wider claims and recommendations regarding the universality of the conclusions of this specific study.

(J) We thank Reviewer #2 both for the positive comments on the quality of the dataset and the interest in the study. We appreciate all feedback and the opportunity to investigate the suggestions that follow. Again, we acknowledge that we did not provide sufficient detail on definitions of flow types (please see our Response D to Reviewer #1) and that this has likely been the source of some of the criticisms raised, e.g., in Comment L143 and elsewhere. Reviewer #2 asks whether the drip waters monitored are a mixture of diffuse and preferential flow. We confirm that this is correct and that this underlies our interpretation. Thus, we believe that we agree with Reviewer #2 that a reduction in the preferential flow component would produce the observed uptick in the isotopic data. We do appreciate that the reviewer has raised specific concerns regarding the exact nature of the features that dictate the water movement to the cave ceiling, e.g., do the fractures extend from the surface to the cave ceiling. We thank Reviewer #2 for bringing this to our attention and hope that by having clarified the definitions and inserted a detailed ‘Site Description’ to the Supplementary Information, we have now provided sufficient detail for the broad audience that this paper is intended for.

Constructive (I hope) Discussion

The arguments and narrative of the paper rely on the reader knowing the literature produced previously by the group and being aware of the conceptual models of recharge processes developed therein. In order to understand the arguments being made I had to look up and study items 11, 26, 30 and 46 from the reference list, plus an additional item not cited in this paper (Mahmud etal., 2018. Hydrology and Earth System Science, 22, 977-988). With the benefit of this background reading, I felt that some of the conclusions drawn in the paper were not fully substantiated by the data.

(K) Please see response to next paragraph below. we have added ref. [8] for completeness.

The fundamental problem I find is that the interpretations of recharge processes in the paper are based on a conceptual model, but the actual processes of recharge and deep percolation have not in fact been monitored. Consequently the conceptual model lacks detail and invokes mechanisms that have not been proven, such as a threshold effect in initiating fracture flow and the continuity of fracture pathways between the base of the soil and the cave. The previous studies make clear the fact that drip sites in one of the caves (Golgotha) have been mapped and monitored in great detail, and that rainfall and evapotranspiration have been measured or estimated in the immediate area, but there are no direct observations of the processes by which rainfall becomes recharge and feeds the drips that in turn precipitate the stalagmites. The Golgotha cave lies in an area of Eucalyptus forest with a dense understory, which probably inhibits direct evaporation from the soil by shading, producing a low ratio of evaporation to transpiration. This is important because while evaporation isotopically fractionates the residual water in the soil and produces higher values of d18O, transpiration does not. The soils are sandy and vary from 0.3 to 3 m thick. Thus some parts of the ground possess a deep store of soil moisture, but others do not and intense or prolonged rainfall may saturate these.

(L) We have added a new figure (new Fig. 6) with water table data from a new site (Lake Cave, 3.6 km from Golgotha) that spans the interval of the cave monitoring data. Fig. 6 shows that the water table response occurs approximately at the time as increased dripping from stalactites with a preferential flow component. This provides data to link preferential flow and recharge to the water table.

We have added a new text section under new sub-heading 'Settings and Approach' (Line 86-105) and a detailed 'Site Description' (see Line 48-118 in the Supplementary Information) to provide additional text to summarise site observations and interpretations from our previous studies including the information above regarding vegetation, evapotranspiration and soil from ref. [9,5]. We have included detail on the findings by ref. [4] that used time lapsed electrical resistivity tomography (TL-ERT) data revisited over the same survey line at different times in the hydrological year to image the water movement from the soil to the depth of the cave. This TL-ERT data supported two phase flow with a zone of preferential movement and deep percolation within a hydrological year.

Regarding the "threshold effect in initiating fracture flow … has not been proven", the studies that followed by ref. [1,10] show that a portion of drip sites exhibit a threshold rise in drip rates followed by slow decline, similar to stream hydrographs, in response to infiltration that is consistent with preferential flow along a fracture or conduit. This is now clearly stated in the third paragraph of 'Settings and Approach' (Line 103-105).

The rock in which the caves lie is a very porous calcarenite. Thus moisture can move downwards through the bedrock as unsaturated flow in a sandy porous medium. In the roof of Golgotha cave the rock displays some fracturing and the development of solution pipes, dissolved presumably by concentrated former flows of recharge water. In Ref.11 Lidar mapping of the cave roof is used to characterise drip points in terms of the presence or absence of a fracture, those without a fracture being fed by flow from the matrix of primary pores in the calcarenite. It is shown in that paper and in Mahmud et al (2018) that drips from fractures have greater variability in their discharge than those from the matrix. References 26 and 46 demonstrate that there are significant offsets of 0.6 – 1 ‰ in the average d18O values between these two types of drips.

A striking feature of the cave roof is that both stalactites and drips are concentrated in roughly circular damp areas of the roof, a few metres across, whereas the intervening areas are dry. This implies that vertical flow through the unsaturated calcarenite must take place in fingers with higher water content, separated by volumes of calcarenite with a much lower water content. As water cannot enter the cave void unless the pore water pressure is at least one atmosphere, only those parts of the cave roof that intersect these fingers are close to saturation and able to drip water into the void. (This applies to fractures as well as intergranular pores). Various factors may assist in concentrating downward flow of moisture into fingers, including bedding surfaces and clay-rich layers in the calcarenite, variations in the soil thickness, and the presence of fractures. The formation of fingers in sandy porous media is easily observed in both lab and field, wherever a wetting front separates a layer of high water content from underlying sand with a much lower water content. The phenomenon arises from the non-linear relationships between hydraulic conductivity, soil water tension (i.e. non-gravitational hydraulic potential), and moisture content in unsaturated sand. Reference 46 contains photographs of one macro-finger of high moisture content which is visible in the cliff wall of the doline outside the entrance of Golgotha cave.

It is important to note that the role of fractures in channelling water to drip points has only been established by observing their presence on the cave roofs. It is not known whether fractures form a linked network across the whole 25 – 40 m thickness of the unsaturated zone above Golgotha cave, or the other caves in this study. Nevertheless the paper under review uses terms such as 'fracture pathways' and 'focussed flow' without defining what they mean or explaining the evidence on which these concepts appear to rest. Reference 30 presents time-lapsed electrical resistivity imaging of the unsaturated zone above Golgotha cave in different seasons. This supports the conceptual model of fingering at macro-scale but isn't detailed enough to image either the cave itself or the meso-scale fingers that are evident on its roof. The electrical imaging does not provide any information on fracturing.

(M) With regards to the statement "the role of fractures in channelling water to drip points has only been established by observing their presence on the cave roofs", this is incorrect. Please see response L second paragraph.

Regarding the TL-ERT study[4], we fully agree with Reviewer #2 that it is not possible to physically determine whether a preferential flow path is comprised of a single fracture or a series of fractures. In fact, this is clearly stated in the TL-ERT study. We do appreciate the reviewer's point that there is uncertainty regarding whether the preferential flow pathways in this host rock are single fractures or a series of fractures and we have added this to our expanded definition (see Response C Reviewer #1). We have added to the detailed 'Site Description' in the Supplementary Information, photographs of fractures and conduits from road cuttings and quarries in the same Spearwood Formation of the aeolinite that the caves are developed in.

Based on the fact that the d18O of drip water shows seasonal cycles, Reference 46 suggests that the residence time between the surface and the cave roof is less than one year. Given that annual recharge very approximately amounts to about 700 mm depth of rainfall and the cave roof at Golgotha is about 30 m down, a one year transit time implies that the average volumetric moisture content of the unsaturated zone must be around (0.7 x 100)/30 = 2%. The porosity of the calcarenite is 30 – 50%. Therefore if the rock in fingers were almost saturated rock the fingers themselves would have to occupy less than one fifteenth to one twenty-fifth of the total rock volume, more if the moisture content of the fingered rock were lower than near-saturation, and/or if the transit time were longer than one year. Because the unsaturated permeability of sands increases non-linearly with moisture content, the overall proportion of fingered rock and its average moisture content will adjust to the flux of recharge being transmitted downwards. Thus, as recharge fluctuates seasonally, the extent and moisture content of fingering will also change. This appears to be broadly supported by the electrical resistivity measurements in Reference 30. If there are long term changes in the amount of recharge because of climate change, the degree of fingering and the moisture content within fingers may also change in the long term. It is important to recognise that all this may occur without needing to invoke the presence of connected fractures as rapid-transit pathways through the whole unsaturated zone, though this is not to deny their proven presence as local pathways at the cave roof.

(N) We thank Reviewer #2 for the suggestion. We agree that saturated rock fingers are a possibility as was first raised by ref. [5]. However, our evidence since then, published in ref. [1,10], no longer supports this interpretation. For example, ref. [10] shows that the threshold responses to infiltration in the drip rate hydrographs could not be caused without preferential flow along fractures or conduits. Ref. [10] also shows that the hydrographs varied in their response despite their close proximity (10-100's cm) indicating individual flowpath responses. Ref. [1] used water isotope evidence that confirmed differences at a small spatial scale which could not result if a finger of saturated rock was the dominant mechanism (further outlined in response below). We have now added this to our 'Settings and Approach' (Line 98-105) and expanded on in the 'Site Description' (Line 96-118; Supplementary Information).

A crucial element in the paper's argument is the observation that high-flow drips have a lower d18O than low-flow ones. The key step is the use of the conceptual model of recharge to account for this difference. Attribution of the low-flow drips to predominantly matrix flow is supported by general considerations of flow through porous media discussed above, with patches of near-saturated rock forming on the cave roof and allowing water to drip. The fact that there are few stalactites between these patches testifies to their stability over the long term. However the high-flow drips occur amongst the low flow ones so the question is, why is their d18O less than that of the low-flow drips which itself approximates average rainfall? The conceptual and numerical modelling in Ref 46 attributes this to a separate reservoir feeding these drips, but the nature and location of such a reservoir is difficult to envisage. In contrast, the present paper appears to envisage fractures as providing a continuous rapid transit pathway from the surface, but one that is only activated by relatively large or intense falls of rain. The reservoir effects of such pathways would be small compared with the fingers of unsaturated flow in which they must be embedded, and there would inevitably be some migration of water from the fracture into a matrix with lower capillary potential. Uncertainty about the lengths of individual fractures and the presence of a percolating network of fractures remains, however.

(O) We confirm that the preferential flow drips occur amongst the drips supplied by the matrix; the reviewer asks why "is their d18O less than that of the low-flow drips which itself approximates average rainfall?" This is described Line 206-208 and the explanation given in ref. [1] which is cited at Line 276-299. There is a clear inverse relationship between rainfall amount and rainfall $\delta^{18}O$ as was correctly cited as ref. [11]. We have also added a new section that shows a small difference in the slope of diffuse versus preferential flow drips on a dD versus $\delta^{18}O$ plot which may support that evaporation of the slower moving diffuse flow water is also contributing to the isotopic difference between flow types (Line 269-273). Please see our response F.

Reviewer #2 refers to Ref 46 (ref. [5]). We confirm that the conceptual model of ref. [5] with preferential flow drips being fed by a separate reservoir is outdated. As outlined in our response E above, ref. [5] was published in 2013 and we've subsequently updated this interpretation and these recent studies were cited to support our interpretation (see Line 98-105). The studies of ref. [3,10] demonstrate that drips with a preferential flow component (via fractures, conduits etc.) occur in close proximity to drips dominated by supply from the matrix. This is further supported by the drip water isotopic data presented in ref. [1] (see response E). With regards to lengths of fractures, we've added new detailed 'Site Description' and images of the host rock to the Supplementary Information.

An important consideration is whether the isotopic difference between high and low flow drip sets has persisted as the general rise in d18O has occurred. This is not apparent from the data in this paper, but Fig. 5 in Ref 26 indicates that it has, with individual members of both groups showing much the same upwards trend. This suggests that the mechanism producing the difference has persisted even as the frequency of near-saturated conditions in soils has declined, and with it the supposed frequency of recharge by large or intense falls of rain with lower than average d18O. This is difficult to reconcile conceptually with the fact that the mean d18O of rainfall has not changed significantly during the period of records. Without details of the relationships of rainfall amounts to d18O, it is difficult to evaluate whether this persistence is compatible with the hypothesis that the lower d18O values in high-flow drips derive from large or intense rainfalls that exceed a threshold for initiation of fracture flow.

(P) Ref 27 (ref. [11]) provides an analysis of rain water isotopes and climate data for the site. In Fig. 5, the distributions calculated for rainfall exceeding increasingly higher thresholds is shown. The distribution of preferential flow drips shows a bias towards higher recharge thresholds. Text in paragraph beginning at Line 276 has been modified to expand on recharge bias and it's effect on drip water. An example of the isotopic response of drip water to activation of flow along fracture pathways that was investigated in ref. [1] is referred to at Line 299.

With regard to the trend present in all drip sites monitored, please see our response to Comment L143 below.

A mechanism that might account for the rise in d18O in both groups of drips is isotopic enrichment of soil water by evaporation. Although the ratio of evaporation to transpiration is likely to be in favour of the latter, it may have changed in the direction of relatively greater evaporation as soils became drier and trees limited their transpiration during longer periods of moisture stress. The residual moisture in the soil at the end of summer is likely to be quite highly fractionated and this would then be mixed with relatively unfractionated water from the succeeding wet season's rainfall. As the threshold for supposed fracture flow is more likely to be exceeded in winter, the O-18 enriched soil water from the previous summer may remain a significant component of the mixture when recharge into fractures occurs. This is certainly very speculative but the idea is testable by comparing d18O and d2H in drip waters and looking for indications of evaporation, especially among samples whose d18O values are relatively high for each site. Inspection of Fig.4 in Ref 26 does not provide support for the idea, although it is noticeable that the fast drips 2B and 2E have lower d18O and higher deuterium excess than the remaining three which have compositions that lie close to the mean values of rainfall. This pattern has been observed elsewhere (e.g. in Grotte de Villars, France) and may be due to additions of condensate from vapour derived by evaporation from soil moisture. In principle such additions are possible in the summer period when the cooler denser air in the porous bedrock will drain into the cave (incidentally raising the cave air CO2 levels) while near the surface air is drawn downwards through warm sandy soil into cooler regions at depth. Because connected fracture paths would transmit much larger local fluxes of air than the matrix, this mechanism might enrich the water in them in O-18.

(Q) We thank the reviewer for the above helpful suggestion. Please see response D to Reviewer #1.

It is clear from this lengthy discussion that in this reader's mind at least there is much uncertainty regarding the possible reasons for the disparity in d18O values between high and low flow drips. I am not convinced that it is a simple matter of a bias towards isotopically light recharge. In view of this uncertainty, is it wise to promulgate the opinion that the rise in d18O observed in the record, and in fast drips as well as slow ones, is merely due to a reduction in the contribution made by a still hypothetical fissure flow? The rise in d18O is an established fact but the mechanism by which it occurs, and its relationship to recharge amount remains too uncertain for the next step in the paper, the warning that "fracture" or "focussed" flow is in danger of ceasing altogether.

(R) We agree that a reduction in the preferential flow component is the most plausible interpretation for the observed uptick in the isotopic data as highlighted by our field evidence in the second paragraph of response L. Please see our response to Comment L143 below also.

One other comment occurs to me having looked at Fig.4 in Ref 26. This shows the records from all four Golgotha speleothems, comprising half of the group of eight discussed in the present paper. All four have records extending back to 1200 CE, yet only one – GL-S4 – is mentioned in the present paper and compared with the Law Dome ice record. The other three fit this record less well. Moreover, three of the four have d18O values between 1150 and 1400 CE that exceed all values in the respective stalagmites since then, and even exceed those of the drip waters and farmed calcite in the present century. Two of the three have high values at the start of their growth, one being GLS4, while the third (GL-S3) has the highest d18O of all just prior to a cessation of growth that lasted almost six centuries. Such patterns of high d18O at start and end of growth of speleothems are common world-wide and are generally correlated with similar changes in d13C of the calcite. This is an indication that the amount of fractionation by degassing is affecting both isotopic systems. Among other factors, greater degassing is promoted by longer residence times between drips. Could this also be a factor affecting the farmed calcite as well as the speleothems in the present study? However, it would not produce the observed rise in the d18O of the drip waters unless there were significant amounts of evaporation at the drip site, and this does not show up in the cross-plot of d18O versus d2H in Ref 46.

(S) Reviewer #2 asks whether a change in the fractionation between calcite and water could explain the isotopic uptick observed in the current study, but also correctly notes that the proposed mechanism would not produce the observed $\delta^{18}O$ rise in drip waters. With regards to a potential change in fractionation, please see response B.

Reviewer #2 notes that only the GL-S4 speleothem record is presented as a paleoclimate record in the present paper. This is correct. All four records and their flow properties are assessed in ref. [1] and it was concluded that "Speleothems associated with large, complex fractures are more likely to show non-linear responses to hydroclimate forcing and more caution should be taken with interpreting paleoclimate information from them. In contrast, speleothems dominated by matrix flowpaths and simple fracture structures are likely to be more suitable" (page 6 ref. [1]). It is shown in ref. [1], and outlined in Line 447-449 of the manuscript, that GL-S4 is "fed by diffuse drip water supplied from the matrix but also by preferential flow along fractures that activate during wetter years" We have now further added (Line 449-451): "The other three speleothems were supplied predominately by matrix flow (GL-S1) and complex fractures (GL-S2 and GL-S3). Thus GL-S4 was considered the most suitable record to assess variability in preferential flow in the longer-term context."

Detailed comments on the typescript

Line 1. Title: This is misleading. There is abundant local evidence of decreasing recharge from independent sources (weather records and calculated water balance, declining water table in two caves, as in Fig.3d, So the speleothem warning is not 'early', though it is timely. As discussed above, the mechanism by which 18O of drip water and calcite might act as a proxy for recharge amount has not been fully established. In addition, the d18O values seen in the last few decades were exceeded in the period 1150-1400 CE in some of the speleothems. I suggest a different title, such as "Speleothem data suggest that current drying trends in Southwest Australia are unprecedented in the last 600 years". This is a more accurate indication of what can actually be shown from the data by rigorous argument rather than loose analogy.

We thank Reviewer #2 for the suggestion. Journal guidelines limit the title to 15 words or fewer and we have supplied a revised title: Caves demonstrate decrease in rainfall recharge of groundwater is unprecedented for the last 800 years. We note that 800 years is correct when the magnitude of the uptick is interpreted from the composite uptick data, i.e., including the drip data that extend the record as shown in the final figure.

Line 10. "shallow karst aquifers becoming disconnected from recharge". It's not clear what this means in hydrological terms. It suggests that dripping ceased or was supplied from some other source than rainfall. Line 10 is in the abstract which is limited by a word count. It is more fully outlined in hydrological terms in the main text. Also please see response to Comment Line 176.

Lines 11-12. "recharge no longer reliably occurring". The paper shows that there has been no cessation of dripping. So recharge is still occurring and has not yet been interrupted. We do not mean to claim that recharge has ceased and to ensure this is understood we have amended to "recharge may no longer be reliably occurring".

Line 12-13. I think this is the main point of the paper. We fully agree.

Lines 13-16. This might be a good idea for drip water monitoring but in the context of this paper it looks (to me) like window-dressing. We thank the Reviewer for their perspective on this. We have deleted this sentence.

Lines 16-18. In what way has the causal connection between rising d18O and water balance or recharge amount been proven? I think there is still an undistributed 'grey box' between the two, so the validation rests mainly on visual correlation with the rainfall record over the last few decades. It would perhaps be useful to explore that more fully. We thank the reviewer for highlighting that this point could be stronger. We have looked at our available data and provide a new figure (Fig. 6 in the revised manuscript) which shows that drip rates from preferential flow influenced sites and the local water table respond to changes in water balance. This is described in our response Line 368-381.

Line 25. Some further references on the sensitivity of groundwater recharge to climate change: Yusoff, I., Hiscock, KM. & Conway, D., 2002. Simulation of the impacts of climate change on groundwater resources in eastern England. In Hiscock, KM., Davison, RM. & Rivett, MO. (eds.) Sustainable Groundwater Development. Geological Society of London Special Publication 193, 325-344. Herrera-Pantoja, M and Hiscock, KM, 2008. The effects of climate change on potential groundwater recharge in Great Britain. HYDROLOGICAL PROCESSES 22 (1), pp.73-86. Herrera-Pantoja, M., Hiscock, KM. & Boar, RR., 2011. The potential impact of climate change on groundwater-fed wetlands in eastern England. Ecohydrology, 5 (4), 401-413.

We thank the reviewer for highlighting these additional references and have included ref. [12].

Line 31. 'Soil water balance' would be a better term to use than "rainfall" as it is the balance of rainfall, evapotranspiration and their effect on the occurrence of the state of field capacity that are critical for production of recharge. We thank Reviewer #2 for the suggestion. The sentence now reads (Line 43-44): "These factors mean that attributing changes in groundwater recharge to the impacts of climate change on rainfall **and the surface water balance** is difficult." The manuscript goes on to discuss the importance of soil water balance in leading to rainfall recharge.

Line 36. In the context of the paper it is important to emphasize that the high permeability is primary, i.e. intergranular. To avoid confusion I suggest removing the word "diffuse" as it adds little to "porous" and in the karst literature the term 'diffuse groundwater flow' refers to non-conduit flow, typically in fracture networks. So I suggest, '…can be porous flow where the host rock has high primary permeability." We agree with the reviewer that our descriptions could have been more accurate and complete. We have now added a new section of text 'Site Description' (Supplementary Information) where the host rock is described as having high primary porosity and permeability. We have also added that it is possible that diffuse flow can arise from other porous media such as sediment infilled conduits or paleokarst. We prefer not to remove the term 'diffuse' (originally Line 36 now Line 49) as this term has been widely used in cave monitoring studies and in the speleothem science community whom are likely to be a large portion of the readership of this paper.

Line 37. "two flow types" Four types of pathway have been mentioned. Are the flow types distinct in these? Unsaturated water flow under a gravitational gradient in fractures, fracture zones and conduits, versus unsaturated flow in a porous medium under the combined gradient of matrix potential and gravity. It looks as if it is the potential gradient that differs, rather than the type of flow. We thank the reviewer for pointing this out. We have re-worded this sentence (Line50-51): "In karst environments recharge is likely to be a mixture of **diffuse and preferential** flow types[10,13]".

Lines 40-41. Speleothems provide evidence of the occurrence of recharge, but give only a relative indication (or none) of its amount. With respect, we have not made the claim that speleothems can quantify amount of recharge. In full, the text reads: "Similar to groundwater, the oxygen isotopic composition ($\delta^{18}O$) of cave drip waters and consequently speleothems, is largely determined by the recharge-weighted $\delta^{18}O$, particularly in water-limited climates[14,15] where groundwater resources are increasingly under pressure. Accordingly, speleothems offer a highly temporally-resolved record of rainfall recharge to groundwater." We believe that it is clear that we are referring to the $\delta^{18}O$ data in speleothems and groundwater here and making the point that both are "largely determined by the recharge-weighted $\delta^{18}O$".

Lines 44-46. Good point which could be placed at Line 35 for greater impact. We thank Reviewer #2 for the suggestion. Sentence moved up as suggested (Line 55-57).

Line 47. The following paragraph jumps rather suddenly to SW Australia. We thank Reviewer #2 for pointing this out. We have inserted a sub-heading between these paragraphs 'SETTING AND APPROACH' which is consistent with other articles published in this journal.

Lines 71. Define what is meant by "focussed flow paths". In the context of the calcarenite aquifer these could be fingered flow in the porous medium, and/or flow along sedimentary discontinuities, as well as flow in fractures. Only the first of these has the effect of 'focussing' the flow – in the sense of convergent flow from a large cross-section into a much smaller one. 'preferential flow' is a widely used term that might be better. We agree and thank the reviewer for pointing this out. We replaced "focussed flow paths" with "preferential flow paths" here and throughout. We have also expanded the definitions given around Line 48-51 and provide further information under new section titled 'Site Description' in the Supplementary Information.

Line 72-73. What is demonstrated in the paper is that the uptick in d18O is unprecedented. The causal link to recharge reduction is rather less certain. We have revised this statement and it now reads (modified text in bold): "We also assess this in the context of the longer (paleo) speleothem record for the region and demonstrate that this **uptick** is unprecedented for the last 800 years**, implying that the current reduction in rainfall recharge is unprecedented over this period**." Line 114-115.

Lines 75-76. Is this a universal effect? The wider applicability of this study to other systems depends on demonstrating conclusively that it is. (I feel that the mechanism isn't fully understood even in the system under study here.) We confirm that these upticks in $\delta^{18}O$ are seen elsewhere in speleothems and have added an example at Line 518.

Lines 77-78. The last part of this sentence did not make sense on first reading, but on returning to it after reading the whole paper several times I think it should be omitted. Although it is incompletely expressed, the argument of the paper uses drip discharge patterns to attribute drip sites to one of two categories, low and high flow sites. Those with low, constant flow are attributed a priori to flow in the matrix whereas those with high and variable flow are regarded as being fed by fractures. The paper goes on to attribute the uptick in d18O to a decline in the importance of the fracture category, This attribution does not validate the identification of the original category with fractures, and to claim it does is a circular argument. (Only the drip characteristics have actually been observed, the attribution is an inference.) We agree that this sentence could be worded better and thank the reviewer for pointing this out. The intention of the sentence was to communicate that the study validates that flow paths impact speleothem $\delta^{18}O$. Please also see our response to Reviewer #2 above where we have outlined the hydrograph and water isotope evidence in ref. [1,10] for identifying drips with preferential (fracture) flow component.

For clarification we have rephrased this sentence to read: "This substantiates the interpretation of speleothem $\delta^{18}O$ datasets as a proxy for hydroclimate, and importantly validates the **role of karst** flow path**s in driving a non-linear response** to **hydroclimate forcing via** control on recharging water $\delta^{18}O$ values."

Line 85-86. The latest rise in MND-S1 starts in the early 1970s, not the 1960s when d18O is falling slightly. The values reached at the end of this record in the early 1990s are lower than those in the 1940s. This last point is also true for LAB-S1 and YD-S2. We thank Reviewer #2 for pointing this out and agree that the sentence is not correct. The point that we are trying to make here is that there is variability in individual records with respect to the timing of the $\delta^{18}O$ rise. We have rephrased this sentence to Line 138-142: "The beginning of **the uptick** varies between speleothem records **which may** be due to different hydraulic response times (i.e. cave flow properties) or speleothem age uncertainty**, although the latter is small (see Methods)**. Nevertheless, the comparison of records indicate that the isotopic rise becomes a common signal by the 1990s." Record YD-S2 has been removed from this dataset.

Line 88. "composite data" – does this refer to the composite record in Fig.3a? As it is an average it represents the common signal but does not prove commonality. Perhaps the phrase 'comparison of records" (as in Fig.2) would be better? We agree and have changed "composite data" to "comparison of records".

Lines 89-91. The drip water data have been converted to equivalent values for precipitated calcite, using a fractionation factor based on comparing drip waters and farmed calcite. This factor is empirical and not the equilibrium one appropriate to the cave temperature. Some explanation of all this is needed, otherwise the comparison makes no sense because the media and isotope scales would normally be different. We agree with Reviewer #2 that the conversion of water to calcite equivalent values is not always straight-forward owing to the uncertainties regarding which fractionation factor to apply. However, in this study we have used the empirical fractionation factor determined from farmed calcites at the same drip sites for the Golgotha speleothems and drip water presented in Figure 2. Please see response B for further explanation as to why we argue this was the most appropriate method.

Lines 93-95. The two references (23, 24) would be better placed after “multiple processes”. Neither of them is specifically about category iv in the list. Moved citations as suggested and added a third citation specifically for category iv (flow paths).

Line 99-100. “recent warming16 ”: does this refer to temperatures on the surface of in the cave? One would expect a lag in the cave temperature that might be several decades behind the changes in external temperature. Therefore 0.27 ‰ is probably an upper bound to the effect of cave warming. (Ref 16 appears to be mainly about rainfall; does it also detail local changes in surface temperature?) We thank the reviewer for pointing this out. This text does refer to external temperatures and we agree that 0.27 ‰ is thus probably an upper bound. As it doesn’t have any consequences for the interpretation (we dismiss the possibility of temperature being a driver) we think it is best not to add further detail on this to the text as it could distract from the main point of the sentence which is that warming would serve to lower not raise $\delta^{18}O$ values as observed.

We thank the reviewer for querying if the correct source was cited here. We have changed the citation to the Bureau of Meteorology climate change tracker website. The Bureau’s data were used in the calculation above.

Line 101-103. This statement is misleading. The empirical fractionation factor between drip water and farmed calcite is stated at Line 247 to be 1.0318 (Ref 26). The equilibrium value at cave temperature (14.6 o C) is 1.0307 and the discrepancy clearly indicates isotopic disequilibrium. In terms of temperature, the Golgotha calcites would indicate a temperature of 9.8 o C if assumed to have been deposited at equilibrium as defined by the Kim & O’Neil equation. Though there have been suggestions that Kim & O’Neil’s experimental data may not reflect ‘true’ equilibrium, they have recently been replicated in a laboratory simulation of stalagmite growth (EL-Shenawy, MI; Kim, ST and Schwarcz, HP, 2020. Carbon and oxygen isotope systematics in cave environments: Lessons from an artificial cave "McMaster Cave''. Geochimica et Cosmochimica Acta 272 ,137-159). This showed that slow degassing from a pool resulted in equilibrium deposition in full agreement with the original 1997 Kim and O’Neil study, whereas deposition from a thin water film fed by dripping onto a ‘stalagmite’ showed disequilibrium and larger empirical fractionation factors. This last appears to be the case for Golgotha cave too. Other recent lab studies confirm this (Hansen, M et al., 2019. Chemical Geology 509, 20-44 and citations therein).

With respect, we don’t agree that the statement is misleading and we prefer not to enter into discussion about the absolute value of fractionation factors, i.e., which values represent ‘true isotopic equilibrium’ nor do we claim that the Golgotha records are completely unaffected by disequilibrium. The point is that the fractionation factors calculated for Golgotha using farmed calcites and drip water (Table 1 in ref. [1]) show that these are similar between drip sites and through time. If *enhanced* disequilibrium, i.e, becoming greater through time, were driving the uptick then the fractionation factor would also be changing. See also a recent publication by ref. [16] for discussion on why fraction factors from lab based studies may not be as representative as those from cave monitoring studies.

Lines 106-7. There are other possibilities, for example the rise in d18O may be driven by a greater proportion of rainfall being evaporated as opposed to transpired, as rainfall has declined and longer periods of moisture stress may have limited transpiration. See “Constructive Discussion” above. We thank Reviewer #2 for raising this point. Transpiration has now been explored in the revised manuscript. See Line 226-234.

Lines 123-124. This is the crucial observation made in the paper. We fully agree.

Lines 129-130. This effect was first noted by JC Vogel and H Van Urk, 1977. Isotopic composition of groundwater in semi-arid regions of southern Africa. Journal of Hydrology, 25, 23-36. The paper is still being cited. Thank you for pointing this out. We’ve added this citation.

Lines 132-135. The statement is true but the argument is not conclusive because all of the three rainfall distributions overlap with both drip types (Fig.4). We thank Reviewer #2 for raising this point and agree that the sentence needs rewording. It now reads (Line 280-287): “Figure 5 shows the distributions of drip water compared with rainwater $\delta^{18}O$ values for these data. Based on the campaign data, which sampled the largest number of drip locations (see Settings and Approach), the distribution mode of diffuse-flow dominated drips (-4.12±0.12 ‰VSMOW) is closest to the precipitation-weighted mean of rainfall (-3.61±0.21 ‰VSMOW), whereas the distribution mode of preferential flow-influenced drips (-4.67±0.06 ‰VMSOW) are 1 ‰ lower than rainfall (Fig. 5). The lower value for the preferential flow $\delta^{18}O$ values is closer to rainfall when only heavier totals are included (-4.55±0.40 ‰VSMOW, daily events >9 mm; Fig. 5).” We have also updated Fig. 5 (originally Fig. 4) with the rainfall distributions plotted on the opposite axes for clarity and also added mean values with uncertainties to support our text.

Lines 135-136. Refs 14 and 26 are based in part on data from the same SW Australian caves as in this study. Is there a danger of circular argument in this sentence? It is correct that the four drip sites in this study form part of the global dataset presented in Refs 14 and 26 (ref. [1,17]). However, these represent four drip sites out of a total of n=163 drip sites in ref. [17] and n=229 drip sites in ref. [1] (n values can be found in results of these studies). As the SW Australian data represent 2.5% and 1.7%, respectively, of these global data we do not agree that there is a danger of the four sites skewing the global dataset in a way that could support a circular argument.

Lines 137-138. “… supporting that rainfall recharge to groundwater beneath these caves occurs primarily via fracture flow.” My italics – why? Are all the other aquifers fractured? This argument should be made in more detail, especially as the continuity of the fracture network in the calcarenite aquifer is unproven. We have changed “fracture” to “preferential” flow here and throughout.

We have a new figure linking discharge from preferential flow drips to water table response (new Fig. 6) and included this isotopic evidence to draw the link between rainfall recharge to groundwater below the caves via preferential flow based on the isotopic evidence (Line 371-381). Please see also the expanded description of the host rock in the Supplementary Information.

Line 143. “… the only mechanism that can explain the magnitude of the d18O uptick…”. The difference in d18O between drip types is not large enough for this. If diminished fracture flow really is the explanation, that would imply that the d18O of the fracture waters must be even lower than the values seen in the high-flow drips, i.e. the latter must be mixtures of fracture and porous pathway waters. That in turn implies greater selectivity of low-d18O events during recharge, but could also explain why the fast-drip d18O has continued to rise. However this explanation would also imply that the slow drips are mixtures too, with the proportion of fracture water declining over time. We thank the reviewer for clearly pointing out that it is the varying contribution of diffuse versus preferential flow to each of the four drip sites that would make a trend in all four drip sites possible. We fully agree and this is indeed how we interpret the field evidence and we apologise if this was not clear in the original manuscript. We did state in the original manuscript that "…recharge is likely to be a mixture…" (*Line 37*) and the term "fracture dominated" (e.g. *Line 128*) and "the contribution of diffuse versus focussed flow to drip water dominates stalagmite $\delta^{18}O$[1]" (*Line 181*) in the text, i.e., it was represented in the manuscript as each drip being a mixture of diffuse versus preferential flow. Please also see our response D above, clarifying that the drip sites are mixtures of diffuse-matrix versus preferential-fracture. Also, please see response C for our expanded justification for the reduction in recharge by preferential flow as being the driver of the uptick.

The interpretation of the flow paths influencing the mean drip $\delta^{18}O$ value at each site and the trend present in each was previously presented in ref. [1] which is appropriately cited throughout. In this previous study the following was stated which is consistent with the points above: "The much larger >1‰ shift in the cave data demonstrates an additional mechanism is amplifying the isotopic response to climatic forcing and is consistent with a reduction in fracture flow, i.e., conditions were insufficient to overcome recharge threshold(s) for regular fracture activation resulting in a isotopic shift towards the matrix-flow end-member." In order to demonstrate this clearly in this manuscript, we have updated Fig. 5 (originally Fig. 4) with the diffuse and preferential flow drip water and rainfall distributions plotted, see Comment Lines 132-135. We have also more clearly defined the definitions supplied in this revised manuscript (see response D).

Line 146 refers to three caves with water table measurements but only two are shown in Fig.3d. Why not show the third? We apologise that this was a mistake carried through from an earlier draft and is now fixed. Fig. 3d has been updated to show the longer water table time series from Jewel Cave and there is now a new figure (Fig. 6; see response L) on which a shorter but more recent water table data from a second cave (Lake Cave) are plotted.

Lines 149-151. I think that this data is neglected in the paper. Is the aquifer at Jewel Cave affected by abstraction? If not, the long term decline is pretty clear evidence that recharge has been less than outflow since about 1980. Because of the difficulty in establishing a convincing causal connection between d18O and amount of recharge, this is better evidence that the latter has declined than the stalagmite record, at least for the last few decades. We thank Reviewer #2 for highlighting this. Please see our response regarding a new figure to address this same point on page 8 (response L) of this document.

Line 168. Might "preferential' be a better word than "focussed"? Yes, thank you. Our apologies for the confusion this seems to have caused during the review process.

Line 176. 'disconnection' is an imprecise expression which the use of quote marks seems to acknowledge. The high-flow drips are still flowing so the preferential flow has not ceased, therefore there is no 'disconnection' from rainfall recharge. This needs much more careful phrasing, especially as the paper goes on to extend the idea to all the aquifers of the Perth Basin, purely on the basis of their similarity in d18O distributions with high-flow drips.

We propose that preferential flow is still occurring but has been reduced due to the regional rainfall decrease. More careful phrasing of this has been included in a new discussion section "Implications for understanding rainfall-recharge to groundwater" (Line 367-383) which is built upon the additional evidence now included in the manuscript (Fig. 1, see Comment Lines 532-534; Fig. 4, see response F; Fig. 5 see Comment Lines 132-135).

Line 180. "The replication of the uptick across the southwest Australian region…" This statement is tendentious; only one cave out of eight is outside the peninsula in the southwest. Its record is one of those in which the d18O values at the end of the stalagmite record are exceeded by earlier peaks. This record (YD-S2) is certainly compatible with the others but its uptick since 1985 must be set against four earlier such rises in the same stalagmite since 1915. This pattern does not replicate what is interpreted in this paper as an event that was unprecedented in the past 800 years. It is a very weak basis on which to claim in Lines 181-182 that "no other mechanism could produce this common response across multiple caves". Record YD-S2 has been removed from the dataset. This section of text has been deleted.

Line 183. "… a robustly-constrained process-based relationship…" I do not agree that the process basis for the rising d18O trend is fully understood and therefore I don't think it should be claimed to be robust. The language here is rhetorical rather than factual. Please see response (R).

Lines 197-200. Comparison with the Law Dome record. Fig.5 shows the actual record from Law Dome, It appears to be negatively correlated with the speleothem record. One has to go to the references to discover that rainfall in SW Australia is also negatively correlated with ice accumulation in Antarctica. Either this should be explained, or Fig.5 should show reconstructed values for SW Australia, to agree with the text here. We thank Reviewer #2 for highlighting this. We have updated Fig. 5 to show the rainfall reconstruction for Southwest Australia based on Law Dome, as well as Southern Australia reconstruction based on multiple paleoclimate proxies.

Line 201. What does "we confirm" mean? It is not true that there are no uptick events in the last 800 years. In Ref 26 Fig.4 GL-S3 shows a very strong one between 1260 and 1360 CE, but it is not "replicated" by two other stalagmites in the same cave, one of which is the only one presented here. This omission could be regarded as misleading. It certainly weakens the statement that the recent uptick is unprecedented. While Reviewer #2 is correct regarding the GL-S3 record presented in ref. [1], we have only included GL-S4 as the other speleothem records (GL-S1, GL-S2 and GL-S3) were predominately supplied by diffuse or preferential flow.

We have added justification for the use of only GL-S4 (Line 449-451): "The other three speleothems were supplied either predominately by diffuse flow (GL-S1) or by complex fractures (GL-S2 and GL-S3). Thus GL-S4 was consideredis the most suitable record to assess variability in preferential flow contribution in the longer-term context."

Lines 202-203. "… current decoupling of rainfall to groundwater recharge…" The data do not support decoupling; dripping continues. It is the d18O uptick which is unprecedented (since 1400 CE). Please see response to Comment Line 176.

Lines 208-209. I suggest insertion of 'the d18O of drip waters and calcite deposited from them in' before "caves", and 'as being' at the end of Line 208. In line 209, "early warning" should be replaced by 'an indication'. The text has been amended as suggested by the reviewer (Line 494-496): "In this study, the $\delta^{18}O$ of drip waters and calcite deposited from them in caves located in the vadose zone, and where recharge can be identified unambiguously as being from rainfall, have provided clear evidence of decreased rainfall recharge to groundwater resources."

Lines 210-211. I strongly recommend that this statement be omitted altogether. The data and its interpretation do not support it. Please see our response to Lines 11-12.

Line 212. Change 800 years to 600 and include the long record shown in Fig.4 of Ref 26. We note that 800 years is correct when the magnitude of the uptick is interpreted from the composite uptick data, i.e., including the drip data that extend the record as shown in the final figure.

Line 217 to 220. This recommendation would only be a useful one if and only if (a) the correct reasons for d18O decline have been identified in this study (which I think is still in doubt), and (b) if most karst aquifers shared the same type of flow in the unsaturated zone as the calcarenites in SW Australaia, which they do not. I think that the whole sentence is best omitted. For (a) please see response R. Regarding (b), the host rock has dual porosity flow and is fractured (see Site Description in Supplementary Information) which are all typical features of karst. This has been included in the recommendation and now reads (Line 515-523): "This study shows that a network of monitored caves in other regions where groundwater is relied upon could serve as a **direct and unambiguous measurement** of reduced recharge, particularly **in karstified carbonate rocks that have flow paths associated with both primary porosity and fractures (e.g. as also observed in an Ethiopian stalagmite** [18]) and widespread rainfall declines are expected this century as a result of human-caused climate change[19]."

Line 246-247. See comment on Lines 101-103. Please see our response to L101-103.

Line 324. Reference is incomplete. Now fixed.

Figure 1. The use of colour shading and contours on the main map replicates information and makes the map harder to read at a glance. Perhaps better to remove the colour shading and the part of the key that refers to it? Figure 1 has been revised with shaded section representing the rainfall reduction anomaly, but the rainfall contours have been removed to improve readability. We believe it is critical to highlight the spatial scale of this rainfall decrease for international readers.

Figure 2: The farmed calcite and dripwater continue the upward trend seen in the stalagmites but in individual cases (GL-S1, GL-S3 and LAB-S1) there is near enough continuity between the two types of records to see definite 'jumps' of 0.5, 0.35 and 0.2 ‰ respectively. Could this be evidence that the fractionation between drip water and speleothem is greater than that between drip water and farmed calcite ? The film thickness may not be the same on both types of surface. Correction for this would not remove the uptick but it would reduce its amplitude somewhat. Only five of the eight speleothem records show an uptick that goes outside the range of previous values in the same specimen. Regarding the robustness of the farmed calcites and drip water conversion, please see response B. Regarding the magnitude of offset between the youngest end of Golgotha stalagmites and the drip water measurement, this can be accounted for by the combination of the sampling resolution of the stalagmite records and the fact that the end of each speleothem record and start of drip water occurs during a trend in $\delta^{18}O$. The milling of speleothem powders was 0.1-0.2 mm averaging a minimum of three years of growth. Thus the $\delta^{18}O$ of the youngest measurement on the speleothem records is an average value during which drip water was rising across multiple years. Thus what Reviewer #2 describes as a 'jump' is really a limitation of the method. We had considered attempting to numerically correct for this using extrapolation techniques but prefer to present the data as is, being the most honest presentation of the data.

Lines 504-506. This seems to imply that the normalisation of the d18O series was to 20th century warming values. That isn't correct is it? Figure 3c. The yellow and orange colours are difficult to distinguish. We thank Reviewer #2 for pointing this out. This was a typo. Full-stop inserted after "All data were normalised by subtracting 1900-1968 mean speleothem $\delta^{18}O$ values."

Figure 4. The paper does not discuss the criteria used to group drips into "fracture" and "matrix" categories. This should be explained. Please see response D

Line 530. A reference to an explanation of the kernel density method is needed. Added to caption (Line 930-932).

Lines 532-534. This suggests that the histograms derive from just two drip sites in the same cave. Is this an adequate basis for extrapolation to the whole Perth basin? I think not. We have updated the distributions to be the data from the five sampling campaigns of flow classified drips described in ref. [1]. Up to 25 drips were sampled in each campaign and thus are a better representation of the full range of drips and their isotopic composition. The point is that the rainfall decrease has affected a large region – please see revised Fig. 1 where we have replaced annual totals with rainfall anomaly to highlight for international readers the spatial scale of this rainfall decrease. The study has shown that the rainfall decrease has resulted in the uptick $\delta^{18}O$ response across multiple caves. The GL-S4 record permits this uptick to be assessed in the longer-term paleo record. In doing so, we interpret that the current decrease in rainfall recharge to groundwater is unprecedented in the last 800 years. There are shortcomings in interpreting changes in a region's hydroclimate based on paleoclimate data from a single point but this is a problem that most paleoclimate studies share. The Law Dome record in Fig. 7 is a reconstruction of southwest Australia's rainfall (van Ommen et al., 2010) and supports that the drying in recent decades is unprecedented for the last 800 years. Our GL-S4 record suggests that rainfall recharge during these recent decades is at the lowest that it has been in 800 years.

Figure 5. (a) this panel compares speleothem and farmed calcite from different drip sites. Given my comments on Fig.2, is this an honest representation? Would it not be better to show the composite record instead of the farmed calcite data? (b) This replicates Fig.4. Could the two Figures be combined? (c) This does not show reconstructed rainfall. Should the scale be changed to do this? (a) We apologise that an error was made on the legend. It should have read "drip water" instead of "farmed calcites". Please see our response to L89-91 should further clarification be required on the use of drip water converted to the calcite equivalent scale. We do appreciate Reviewer #2's suggestion to add the composite record to this figure and have done so. We prefer to leave the drip water plotted as it is from the same drip site as the paleo record. The agreement between this and the composite record may alleviate concerns with respect to fractionation factors used. (b) We appreciate the suggestion but point out that although the same data are being represented, they are plotted on the VSMOW scale on Fig. 4 (now Fig. 5) and VPDB scale on Fig. 5 (now Fig. 7). (c) See response to Comment Line 197-200.

Supplementary Material Supplementary Note Lines 2-3. This information should be in the main text, with an indication of the fact that as a host rock, very porous calcarenites are untypical for cavernous aquifers which tend to be in low-porosity, well-cemented limestones with fractures dominating their hydraulic conductivity. The host rock is now stated in the second paragraph of Settings and Approach (Line 87-92).

Lines 12-13. This information should also be mentioned and discussed in the main text. Fractionation of soil moisture has not really been discussed at all and could be a contributing factor to the d18O increase. Please see Response E to Reviewer #1.

Supplementary Table 1. The information on vegetation and on soil thickness and its variability over each site should be mentioned in the main text. Thank you for this suggestion. We have moved Supplementary Table 1 to the main text where it now appears as Table 1 (former Table 1 is now Table 2).

Supplementary Figure 4. This has three panels but the caption refers to a fourth and does not match the Figure. Once again the Law Dome record is said to be a record of reconstructed rainfall in SW Australia. It is not; the scale should be changed if it is to be treated as such. We thank Reviewer #2 for pointing out the problem with the caption. The reference to a panel showing the drip water distributions was superfluous and has been removed. Regarding the axis label for the Law Dome record, please see response to Fig. 5 panel c above.

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12th Apr 23

Dear Dr Priestley,

Your manuscript titled "Caves demonstrate decrease in rainfall recharge of groundwater is unprecedented for the last 800 years" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of Manuscript COMMSENV-22-0366-T entitled "Caves demonstrate decrease in rainfall recharge of groundwater is unprecedented for the last 800 years".

I appreciate authors' careful consideration of the review comments review #2 and I made, and extensive changes/edits resulted.

I am satisfied with the response to the most points I discussed in my original comments. Although I remain skeptical of the regional scale of the patterns of d18O uptick: In Fig. 2, Crystal cave only showed accelerated uptick after 2005, Labrynth cave showed continuous upward change since 1910s with an abrupt acceleration around and after 2000s, Moondyne cave's most recent record (at ~1995) is lower than the values from 1940-1950 with no confirmation of any uptick, the GL-S2's uptick is only confirmed in the farmed calcite record instead of speleothem record. The difference in these time series was not adequately discussed.

With that being said, the manuscript contains important data set, thoughtful and extensive technical discussions and significant implications for understanding the climatic changes in the area. I am ready to recommend publication for the manuscript in its current form.

Reviewer #2 (Remarks to the Author):

I was Referee #2 on the original version of this paper and wrote a long and very detailed review with much constructive citicism. My intention was to help the authors to improve the paper which in that earlier version I felt over-stretched their interpretation of a very solid data set that is backed up by many years of cave monitoring. The authors have replied to my criticisms in detail, adopted many of them and given reasons for rejecting others. I think the paper has been improved as a result of these revisions and also from the revisions made in response to Referee #1. The arguments are now much more rigorously stated and the conclusions follow from them. Apart from some minor copy-editing, I believe the paper to be suitable for publication in its revised form.

We thank the two reviewers for their time and thoughtful comments that helped us to improve the manuscript.

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We agree with the reviewer that the timing of the caves/speleothem response varies. This variation is to do with the unique flow path properties at the site as outlined in Lines 259-260. *'Each speleothem in our composite record will have unique flow path properties and length, yet as with the drip water, all are likely influenced by a mixture of diffuse plus additional preferential flow.'* Nevertheless, fewer preferential-flow recharge events have resulted in a drift towards the higher isotopic value of the diffuse-flow dominated drips in the regional composite record overall.

With that being said, the manuscript contains important data set, thoughtful and extensive technical discussions and significant implications for understanding the climatic changes in the area. I am ready to recommend publication for the manuscript in its current form.

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Minor copy-editing changes have been undertaken as outlined in the track-changes version of the manuscript. We thank the reviewer for recommending the manuscript for publication.